

Is Science Advanced by Deceit?

A QUESTION AND A CRITICISM

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IS SCIENCE ADVANCED BY DECEIT?*

Not quite two centuries and a half ago, a writer whose name both in science and literature is linked to the immortality of genius, found himself engaged in controversy with a great religious order of his Church. In a series of letters, the literary merit of which has never been surpassed, he boldly charged the Jesuit casuists of his time with practical subversion of the foundation principles of Christian morality. But no charge of Pascal has so clung to reputation as that pertaining to the simple virtue of truthfulness. "How, for instance," he asks, "may a man avoid telling a lie when at the same time he is anxious to induce belief in what is false?" In such a case, he tells us, the Jesuit writer Sanchez lays down the doctrine that "it is permissible to use ambiguous terms, leading people to understand them in another sense from that in which we understand them ourselves."† This is the practice and doctrine of Equivocation. But if no equivocal terms come to mind or are available, what then may be done? In such a case one may take refuge in the practice known as Mental Reservation. Thus, says Sanchez:

"A man may swear that he never did such a thing, even though he actually did it, meaning within himself that he did not do it on a certain day, or before he was born, or understanding any other such circumstance, while the words he employs have no such sense as would discover his meaning. This is very convenient in many cases."

It must be said that the Jesuit order has always denied its responsibility for this kind of teaching, even though it was promulgated by casuist writers belonging to the Society.

* From Senate Document No. 78. Fifty-fifth Congress. 1899.
Revised.

† Pascal's Provincial Letters, No. IX.

Certainly, the practice of equivocation and mental reserve is no modern invention, but is as old as the race. Diplomacy so often makes use of words in a double sense that Talleyrand declared language invented to conceal thought. There are nations so imbued with mendacity that they have lost the confidence of their fellow-men. For, however productive of gain they may seem at first, duplicity and deceit have their drawbacks. No man, and no society of men, convicted of habitual resort to the practice of mental reservation or equivocation can permanently retain the trust and confidence of society. And the purpose of this paper is to ask whether this ignoble practice has not come to be, along certain lines, a part of the practical policy of certain scientists in their relations with the public? Is Science advanced by duplicity and honored by deceit?

Let us admit at the outset that this practice of equivocation is by no means of universal or even general adoption among men of science. With the great majority of those whose object in life is to ascertain truth and to promulgate it, there is, happily, no temptation to depart from the strictest veracity. Scientific researches are, for the most part, heartily encouraged by the spirit of our age. Nobody questions the moral right of the geologist, the chemist, the botanist, the electrician, or the astronomer to follow lines of research in any direction desired. Their task is an honored one. It is only when we come to that department of scientific investigation which deals with the phenomena of life that questioning murmurs arise. The morality of a practice engaging the time and energy of a large body of scientific men is questioned, impugned, or denied. There are charges of cruelty, and of a pitilessness which is closely allied to vice. Human nature would be different from what it is, if the men engaged in the habitual practice of vivisection as a means of earning their daily bread could remain unmoved and indifferent. What is the reason, they ask,

that the world should manifest such special curiosity regarding the methods of the physiologist or pathologist? Why should it be asked that their laboratories be made subject to State inspection any more than the observatory of an astronomer? Why should they be obliged to report what they do with dogs, any more than the chemist what he does with his drugs or the geologist with his specimens? The professional vivisector may come to be indifferent to the sight of suffering in an animal; but apathy ceases when he is charged with a vice, and when those whom he has met in society decline to recognize him on the public street.

What shall he reply to such charges? Shall he boldly deny the existence of anything approaching cruelty? How is it possible, against overwhelming proofs to the contrary?

"I recall to mind," says Dr. Latour, "a poor dog, the roots of whose vertebral nerves Magendie desired to lay bare, in order to demonstrate Bell's theory, which he claimed as his own. The dog, mutilated and bleeding, twice escaped from the implacable knife, and threw its front paws around Magendie's neck, as if to soften his murderer and ask for mercy. I confess I was unable to endure that heartrending spectacle." In the documents laid before Congress by various scientific bodies, the name of Magendie is always mentioned with respect, but we venture to say that there is not a scientist in Washington whose name stands as high as that of the late Thomas Henry Huxley, who declared in the report of the Royal Commission: "It is not to be doubted that inhumanity may be found in persons of very high position as physiologists. We have seen it was so in Magendie." Without changing the definition of the word "cruelty," it is impossible in the face of evidence to claim for vivisection, exemption from its stain.

Shall the whole truth about vivisection be freely admitted? That is not an unreasonable demand. We doubt if it would lead to that utter condemnation of the whole practice which

so many scientists seem to fear. But what if statement of the "whole truth" would only intensify the demand for reform? Dr. Klein told the Royal Commission the whole truth in regard to his own practices, and doubtless has regretted ever since his unexampled veracity. One line of defense remains, but we may be sure that it is one to which no man of scientific training ever consciously resorted without loathing and self-detestation. It is the practice of Exaggeration, Equivocation, and Mental Reserve. To the world at large they may seem to deny every charge of cruelty and uselessness, and may have their denials indorsed and supported by the principal scientific bodies of the United States, if only they will adopt the maxim laid down by the Jesuit casuist nearly three hundred years ago, declaring that when one desires to avoid telling a lie, and yet induce a belief in what is false, "*it is permissible to use ambiguous terms, leading people to understand them in another sense from that in which we understand them ourselves.*"

Has this been done? In the defense of the unlimited vivisection of animals, is it true that the names of scientific men of the highest repute may be found attached to statements and denials of charges which apparently were meant to be understood by the general public in a sense contrary to the truth? Worse even than this, has equivocation been used in appeals made to the Congress of the United States solely to hinder and prevent any possible legislation on the subject of vivisection? We propose to examine a single document—Senate Report No. 1049—and to point out some of the many misstatements, evasions, and exaggerations therein made by scientific men and scientific societies regarding—

I. The alleged painless character of inoculation experiments.

II. The extent to which animal experimentation is painless, because anæsthetics are employed.

III. The denial of any cruelty in the practice of Vivisection and the value of such denial.

IV. The exaggeration of benefit to Humanity.

Taking these topics in their order, let us see whether misstatement exists, and if it is due to ignorance or design.

I. Are Inoculation Experiments Painful?

On the evening of April 22, 1896, the MEDICAL SOCIETY OF THE DISTRICT OF COLUMBIA met, and, without a dissentient voice, adopted a memorial to Congress in opposition to any regulation of vivisection. Therein they say:

"As a matter of fact, anæsthetics are habitually administered in experiments which involve an amount of pain worthy of consideration; but they are not considered necessary in trifling operations, such as the administration of a hypodermic injection or the vaccination of a calf." (p. 129.)

We could hardly have more emphatic assurance of the universal use of anæsthetics, except in trifling operations, than is here given on the good faith of the Medical Society of the District of Columbia.

The NATIONAL ACADEMY OF SCIENCES also unanimously assures Congress that—

"In modern laboratories anæsthetics are always employed, except when the operation involves less suffering to the animal than the administration of the anæsthetic, *as in the case of inoculations*, or in those instances in which the anæsthetic would interfere with the object of the experiment." (p. 128.)

Here, too, we have the most explicit assurance that if anæsthetics are omitted in inoculation experiments it is only because the pain is too trivial to make it worth while to use them. Are these assurances the truth, or are they, on the contrary, an ignoble equivocation?

What is an "inoculation?" In medical works it is defined as "the insertion of virus into any part of the body in order

to communicate a disease." An experiment made by means of inoculation means, therefore, that the virus, the poisonous germs of some particular disease, such as cholera, yellow fever, tuberculosis, or rabies, has been inserted—usually by means of a hypodermic needle—into some part of the body of a living animal, beneath its skin, into the abdomen or the chest, within the eye, or upon its scraped surface. When the writer was at Calcutta, in India, a few years ago, they were inoculating monkeys with the venom of the cobra in a series of experiments that, after all, came to no practical result. Thus, in the *Journal of Physiology*, Sewell, of Michigan, tells us of inoculations made by him with rattlesnake poison, using pigeons as subjects, and recording that the head rests on the floor, the mouth open, the respiration gasping, and the body convulsed.* Thus Ernst, of Harvard, inoculated with the virus of rabies, by means of trephining the skull, some thirty-two rabbits, the animal becoming so changed in its natural disposition that from being "lively and affectionate, it becomes dull, sluggish, and even fierce," and so losing the power of swallowing that at first he supposed that they died of starvation.† Thus Cheyne, of England, tells us that "on many occasions I have inoculated portions of synovial membrane and pus from strumous joints, subcutaneously or *into the anterior chamber of the eye*, in rabbits and guinea pigs, and have invariably produced typical tuberculosis by this means." The animals in some of his experiments were not killed for weeks.‡ Thus Klein, of London, the scientist who affirmed that, for himself, he had "no regard at all" for the animals he vivisected, tells us of experiments made by inoculating the eyes of cats with the virus of diphtheria. He records that after such inoculations, in one case "the disease set in with great

* *Journal of Physiology*, Vol. VIII, p. 206.

† *Jour. Med. Sciences*, April, 1887.

‡ *British Medical Journal*, April 11-18, 1891.

intensity," both eyes being closed and the animal living until the seventeenth day; that in another cat, which lived for fifteen days, a "deep, crater-like ulcer" had formed, the eye being much congested, swollen, and coated with purulent matter; that in a third cat the disease steadily increased until the middle of the third week, although great congestion began on the fourth day, and the experiment lasted till the eye became perforated.* Do we need to tell anyone that such "inoculations" were by no means "trifling" in the amount of pain they caused?†

While in Paris recently the writer visited the Pasteur Institute and was shown over the establishment. There were over 2,000 rabbits awaiting their fate. But neither the great number of victims to research, nor the vast iron cage with the dogs tearing at their chains so impressed memory, as the scores of rabbits lying in their compartments slowly dying, the result of inoculations which the AMERICAN ACADEMY OF SCIENCES informs Congress "involved less suffering than the administration of an anæsthetic!"

One inoculation experiment of this kind has for us a special and peculiar interest. In the AMERICAN JOURNAL OF MEDICAL SCIENCES for July, 1882, Dr. George M. Sternberg, now the Surgeon-General of the United States Army, gave to the world the startling discovery he had made through experiments in blood poisoning. "I have demon-

* Sup. to XIXth Annual Report, Local Gov. Board, 1889-1890.

† "Inoculations into the anterior chamber of the eye of rabbits and other animals have frequently been practised, and offer certain advantages in the study of the local effects of pathogenic organisms. . . . Inoculated animals should be carefully observed, and a note made of every symptom indicating departure from the usual condition of health, such as fever, loss of activity, loss of appetite, weakness, emaciation, convulsions, dilated pupils, the formation of an abscess, or a diffuse cellulitis extending from the point of inoculation."—A MANUAL OF BACTERIOLOGY, by George M. Sternberg, M.D., Surgeon-General U. S. A., pp. 97-99.

strated," he tells us, "by repeated experiments, that my saliva . . . injected into the subcutaneous connective tissue of a rabbit, invariably produces death, usually within forty-eight hours. . . . I think I am quite safe in stating that I have repeated the experiment at least twenty-five times with my own saliva. I beg those who undertake to repeat my experiments to observe that *my* saliva produced results recorded. The saliva of four students, residents of Baltimore, gave negative results. . . . In my experiments the rabbits were commonly found dead or dying on the second morning after inoculation. The constant pathologic lesion found by me was a diffuse cellulitis or inflammatory oedema, extending in all directions from the point of injection. The spleen was usually greatly enlarged; the liver was usually dark in color and gorged with blood."

In his "Manual of Bacteriology," Dr. Sternberg claims to have discovered through these experiments a microbe, which he tells us, is now supposed to be concerned in the production of one form of pneumonia. Whether or not this theory is correct, the treatment of pneumonia has remained precisely the same, since this peculiar discovery was made. But what we wish especially to emphasize is the fact that an inoculation experiment, so far from "involving less suffering to the animal than the administration of an anæsthetic," may produce severe and prolonged anguish for days and weeks. Was this fact known to the members of the scientific bodies whose statements to the contrary I have quoted? Every man knew it. How, then, could the MEDICAL SOCIETY OF THE DISTRICT OF COLUMBIA dare to assure the Senate of the United States that an experiment of this character was "a trifling operation," or the NATIONAL ACADEMY OF SCIENCES declare that they "involved less suffering to the animal than the administration of the anæsthetic?" Well, until somebody "rises to explain," we

can only speculate. Let us imagine this memorial brought up for adoption before one of these learned societies. Suddenly a member finds himself on his feet. "Mr. President, I do not see how I can give my vote for that memorial as it stands. Every one of us present to-night is aware that an inoculation experiment involves far more suffering to the animal, as a rule, than the administration of the anaesthetic; that, sometimes, it means prolonged and extreme pain; and yet we, as a society, are assuring Congress and publishing to the world, upon our honor as scientific men, that in this class of experiments anaesthetics are not used because the pain is so trifling!*" That, sir, is a falsehood; and I can not vote for a lie." Then, we may fancy some sturdy vivisector, who perhaps drew up the memorial, rising to reply. "Mr. President, this is a matter of more than ordinary importance. At any cost, we must prevent the bill before Congress from becoming a law. Nobody has asked us to define what we mean *in the laboratory* by an 'inoculation experiment.' Suppose, for the present purpose, we define such an experi-

* A typical instance of equivocation, apparently, may be found in the use made of a quotation from a letter by Surg.-Gen. Sternberg, in the "Memorial from the Representatives of Medical and other Scientific Societies of Washington," printed in Senate Document 107, Fifty-fifth Congress. The italics are as in the original, and their purpose is but too evident:

"The experiments which have been conducted at the Army Medical Museum since I have been Surgeon-General of the Army and, so far as I am informed, previous to that time, relate principally to the cause and prevention of infectious diseases, and to the results of disease processes (pathology). *These experiments do not call for any painful dissections, but consist in the subcutaneous inoculation of cultures of various pathological bacteria, etc.*"

This is signed by Dr. S. C. Busey, Dr. Sternberg, D. E. Salmon, (a veterinary surgeon), and others, and they affirm that it "applies as well to other Government laboratories in this city, where biological research work (vivisection) is conducted." Could anything be plainer than the inference it was evidently designed that the Senate should draw from the words so carefully italicized?

ment as *the prick of the needle by which the virus is inserted* into the tissues. That, certainly is 'a trifling operation'; and I think, with this definition in his mind, even our moral young friend can vote for the memorial. There is no doubt that Congress will accept what we say as the truth, if only we are unanimous." Perhaps such debate never occurred, but only on some such hypothesis is it conceivable how men of science, without a dissenting voice, could give assurances so false.* Even in its best aspect, it was an equivocation. Was it honorable dealing with the National Legislature? Was it in harmony with the ideals of Science? Rather, was it not in perfect accord with the maxim of Sanchez, that when one is desirous to induce belief in what is false, "*it is permitted to use ambiguous terms, leading people to understand them in another sense from that in which we understand them ourselves?*"

* In his Presidential Address in the Section of State Medicine at the last Annual Meeting of the BRITISH MEDICAL ASSOCIATION in August, 1899, Dr. George Wilson, LL.D., probably the leading authority in Great Britain upon Preventive Medicine, made the following indignant reference to these ignoble equivocations :

"I boldly say there should be some pause in these ruthless lines of experimentation. . . . I have not allied myself to the Anti-vivisectionists, but *I accuse my profession of misleading the public as to the cruelties and horrors which are perpetrated on animal life.* When it is stated that the actual pain involved in these experiments is commonly of the most trifling description, *there is a SUPPRESSION OF THE TRUTH*, of the most palpable kind, which could only be accounted for at the time by ignorance of the actual facts. I admit that in the mere operation of injecting a virus, whether cultivated or not, there may be little or no pain, but the cruelty does not lie in the operation itself, which is permitted to be performed without anæsthetics, but in the after-effects. Whether so-called toxins are injected under the skin into the peritoneum, into the cranium, under the dura mater, into the pleural cavity, into the veins, eyes or other organs—and all these methods are ruthlessly practiced—*there is long-drawn-out agony.* The animal so innocently operated on may have to live days, weeks, or months, with no anæsthetic to assuage its sufferings, and nothing but death to relieve."

II. *Are Anæsthetics so used in Vivisection as Completely to Abolish Pain?*

We propose to show that statements, carefully calculated to convey such an impression, were made to Congress for the purpose of influencing legislation; that such impression is absolutely false, and that these statements are entirely in accord with the doctrine of Equivocation.

The Joint Commission of the Scientific Societies of Washington, in their memorial to Congress, asserts that "those engaged in research work . . . may be trusted to conduct such experiments in a humane manner, *and to give anæsthetics when required to prevent pain.*" (p. 130.) Here is a distinct implication that whenever "anæsthetics are required to prevent pain" they are given; and yet every member of the commission who knew anything whatever about vivisection must have known that such meaning of their words could not possibly be true.

A year later apparently the same body, but now styling itself a "joint committee" of various local societies, presented another appeal to Congress, stating: "It is the uniform testimony of those who have had the best opportunities for obtaining reliable information on this subject that in the pathological laboratories in this District, and in the United States generally, anæsthetics are habitually employed for the relief of pain, *whenever it is practicable to give them*, and when the amount of pain involved is such as would call for the administration of an anæsthetic, if the operation were to be performed upon a human being." (Senate Doc. 107, p. 22.) The ASSOCIATION OF MILITARY SURGEONS OF THE UNITED STATES adopted without alteration the memorial of the AMERICAN MEDICAL ASSOCIATION, assuring Congress that "anæsthetics are *habitually administered to animals subjected to painful experiments*" (pp. 131, 132); the MEDICAL SOCIETY OF THE DISTRICT OF COLUMBIA affirms that "anæsthetics are *habitually administered*

in experiments which involve an amount of pain worthy of consideration (p. 129); the NATIONAL ACADEMY OF SCIENCES declares that "the suffering incident to biological investigations is trifling in amount," the NEW HAMPSHIRE MEDICAL SOCIETY asserts that "anæsthetics are habitually administered to animals subjected to painful experiments;" and, finally, the Surgeon-General calls for proof that "those engaged in experimental research do *not* administer anæsthetics to the domestic animals when they are subjected to painful experiments in this District" (p. 125).*

And now, bearing in mind that each of these statements was drawn up by a man of science, trained to the use of accurate expression, and that it was put forth solely to influence Congress against legislation, what is the meaning that a plain man, unused to the subtleties of evasion and equivocation, would find in the passages here quoted? It is doubtful if he notes at first glance that nearly all these assertions are purposely indefinite, and that nowhere is it precisely stated that anæsthetics are effectively used, but only that they are "habitually" administered. What would seem clear to the average man is this: that some of the most eminent scientific men in the United States give their word of honor to the National Legislature that anæsthetics are so given in animal experimentation as practically to annihilate pain, or, if any pain be felt, it is so slight, so "trifling in amount," so similar to that which we endure every day without a thought of anesthesia, that it is not "worthy of consideration." That is the inference which, apparently, it was intended that members of Congress should draw from the statements quoted. And that inference is false.

*The proof is furnished by the Surgeon-General himself. His saliva-experiments were certainly without anæsthetics, and as certainly productive of pain. Unless otherwise specified, all references are to Report 1049.

The exact truth in this matter was perfectly well known to every member of these distinguished societies.

FIRST. The effectual administration of an anæsthetic so as to abolish pain is, as a rule, utterly impracticable in that great class of inoculation experiments to which attention has just been called. Anybody can see that you can not insert virus into the eye or the abdomen of a cat, for instance, and then stand over it night and day administering an anæsthetic; the thing is never even attempted. When Surgeon-General Sternberg demonstrated by experiments upon over twenty-five rabbits his immortal discovery that his saliva, injected beneath the skin, set up all the symptoms of the severest blood poisoning, he certainly did not give them anæsthetics during the entire period of their torment, for he tells us they were "found dead or dying" the second morning after inoculation. But demonstration of the point is quite needless; the facts are admitted. Dr. Woodward, the health officer of the District of Columbia, has stated that "most of the experiments in bacteriology (which includes nearly all of the vivisection done in this District), and a very large proportion of those for other purposes, require that the animal shall be kept alive sometimes for weeks after the effect of the anæsthetic has passed off" (p. 124).* We are therefore indebted to him for revealing, that in the experimentation which goes on in this District, a large majority of the animals must be kept alive for a considerable time.

SECOND. In a large number of other experiments upon living animals, some of them involving prolonged and extreme pain, it is practically impossible to relieve suffering by anæsthetics, unless it be during the brief preliminary cutting operation, when that takes place. In the experi-

*Does Dr. Woodward mean to imply that in "experiments in bacteriology" anæsthetics are administered? Such is the impression conveyed by the above quotation.

ments of Luciani on the starvation of dogs; of Colin, in freezing animals alive; of Chauveau, who tells us that he "consecrated" some eighty horses and asses to experiments on the spinal marrow, producing "intense" and "most violent pain;" in experiments on the reflex action of sensory nerves; in experiments connected with the glandular secretions; in experiments with certain poisons and drugs; in many experiments upon the heart and the circulation, and, in short, whenever the evidence of pain is important to the investigation—complete and genuine anæsthesia throughout the experiment is quite impossible. There are many experiments in surgery where complete anæsthesia can not be maintained. You may, indeed, confer some mitigation of pain by the use of narcotics, such as morphia and chloral, but neither of these is an anæsthetic. As the great experimenter, Burdon Sanderson, has said, "You cannot produce inflammation in an animal, and maintain a state of anæsthesia during the whole process."

THIRD. In addition to these, there are various other experiments, which, if done at all (and their utility is very questionable), must be done under the influence of *curare*, a poison which simply makes the victim incapable of the slightest muscular movement, although conscious of what goes on about it and sensible to every pang.

"An animal under its influence," says Professor Holmgren, the professor of physiology at Upsala University, "it changes instantly into a living corpse, which hears and sees and knows everything, but is unable to move a single muscle; and under its influence no creature can give the faintest indication of its hopeless condition." This venom is, he says, "*the most cruel of poisons.*"* The French vivisector, Claude Bernard, tells us that it "destroys the power of movement, but permits sensibility to exist;" that the "cadaver one has before him hears and com-

* Holmgren's Physiology, p. 231.

hends what goes on about him, *and feels whatever painful impressions we may inflict.*" In a memorial issued last year against legislation, a writer is quoted as stating that "it has never been claimed by any scientific man that it (*curare*) is an anaesthetic." But it is used in every laboratory in America where vivisection goes on to any extent, and one of the principal government vivisectors,—who is not a physician but an experimenter,—Charles Wardell Stiles, insists in his statement to Congress (p. 104) that its use "is a point which should be left entirely to the investigators."

To illustrate its use in laboratories, let us examine the experiments of Dr. H. G. Beyer (a Government employee at the United States National Museum), made upon a large number of dogs. Morphia being administered, the animal is fastened in a "dog holder," tracheotomy performed, a vein dissected out, and "about half a dram of a one per cent. solution of *curare* is injected, after which artificial respiration is begun." The animal is now as solidly fixed to the table as if it were chained, though entirely sensible to pain, and conscious of whatever goes on about it. We need not go into all the details of his experiments—the dividing of nerves, the dissecting out of arteries, the insertion of cannulas, until finally "the whole front and sides of the thorax are cut away and the right subclavian artery dissected out and tied."* They are mentioned only to show that animals, twenty-five or thirty in number, may be slowly dissected alive without anaesthetics; that their death under *curare* may be accompanied, as Claude Bernard puts it, "by sufferings the most atrocious the imagination of man can conceive;" that all this may be done by one of the paid servants of the United States, and yet the Medical Society of the District of Columbia can soberly assure Congress that "as a matter of fact, anaesthetics are habitually administered

* American Jour. Med. Sciences, April, 1887.

in experiments which involve an amount of pain *worthy of consideration!*" No wonder an English experimenter once declared that "anæsthetics do more to lull public opinion than to mitigate animal suffering."*

And now, why was the truth concealed from Congress in this matter of anæsthetics? If, in so much of animal experimentation it is impossible to give complete immunity from pain, why was not the fact admitted? The reason is not difficult to guess. To admit that in a vast number of cases the practice of vivisection as carried on to-day necessarily implies torment, would be to admit the reasonableness of some measure of State inspection and control. Might not that admission be avoided? In one way only. With juggling of words it might be possible to conceal the truth. Unfortunately for the true interests of Science and for the honor of those who assume to speak in her behalf, that course of equivocation was followed out.

* In her statement before the Senate Committee, February 21, 1900, Dr. Mary Putnam Jacobi furnished an additional illustration of the baneful influence of vivisection upon the sense of accuracy and the capacity for stating facts. Referring to the writer she says:

"He does not seem to know as much about the dormitive powers of opium as did the doctors of Molière, and severely condemns Dr. Beyer for an experiment on artificial respiration, because morphine was employed instead of ether or chloroform."

A lady with the experience in vivisection which Dr. Jacobi has enjoyed must know perfectly well that (as Claude Bernard has shown in his "*Leçons de Physiologie opératoire*," p. 115) morphia is *not* a true anæsthetic, whatever might have been the opinion of Molière's physicians two hundred years ago; that the above reference to Beyer's vivisections was to illustrate the use of *curare*, not morphia; that Beyer did not make "an experiment on artificial respiration," but thirty to forty experiments on different animals by exposing and isolating the heart; that the "whole front and sides" of a dog's chest are never "cut away" simply "for an experiment on artificial respiration," and that the above criticism is directed not to cruelty but to falsehood.

III. *Is there any Cruelty in Vivisection?*

Within the past hundred years the ethical ideals of civilization have so far advanced that cruelty to animals, so long a matter of indifference, is to-day regarded as the manifestation of depravity and vice. To the charge of cruelty, therefore, the American vivisector is justly sensitive; and his sensitiveness finds frequent expression in the various memorials made to Congress. Thus the MEDICAL SOCIETY OF THE DISTRICT OF COLUMBIA states with some caution that "so far as we know no evidence has been adduced that *cruel* and unnecessary experiments are being performed in this District." One can not withhold admiration for the diplomacy which does not deny the fact, but only the lack of evidence pertaining to the present time and present place. The CHEMICAL SOCIETY of Washington declares that those who ask for legislation have not been able "to show a single instance of *cruel* experiments conducted in the District of Columbia" (p. 138). The ENTOMOLOGICAL SOCIETY of Washington affirms that it knows of "no *cruel* experiments which have ever been performed in the District of Columbia by any of our colleagues," whatever that may mean. And, finally, the ASSOCIATION OF AMERICAN PHYSICIANS, in a memorial to which are attached the names of the leading vivisectors of America, asserts that "we have been unable to learn that there has been a single instance in which abuse has been made of the practice of animal experimentation in the Government laboratories, the medical schools, or the universities of the District of Columbia" (p. 136).

Are these statements true? They would not be equivocations if in some sense they were not the truth. To the average man they appear to deny in the most emphatic manner the implication that any cruel experimentation ever

occurred in the District of Columbia. Wherein lies the possibility of equivocation? In the definition of the word "cruelty." That word has one meaning for the general public, but an entirely different significance for the vivisector. It is very easy to assert, as these societies have done, that no cruel experiments occur in the District of Columbia, simply because *as cruelty is defined by the professional vivisector, it is practically impossible for him to perform a cruel experiment.*

Let us study a case of what a man of unscientific training might naturally be inclined to stigmatize as a cruel experiment. Suppose that a member of the NATIONAL ACADEMY OF SCIENCES desires scientifically to test the strength of maternal solicitude and affection in a dog. For this purpose he chooses a little spaniel, which for the first time has become a mother. Is it possible to make the animal forget her offspring? In the seclusion of his Washington laboratory he applies in the most scientific manner every known method of inducing extreme agony—lacerating the flesh, irritating the nerves—and yet in spite of every torture inflicted, the creature continues to manifest maternal solicitude. Is there no way to increase its physical torment, and at the same time, to touch the intelligence with despair? As a final resource he cuts off its breasts, so that it can no longer nurse its offspring; and yet, wonderful to relate, the mother-love persists; and the little animal, dying in an agony which it cannot comprehend, unceasingly licks its young! Perhaps you fancy that such an experiment never occurred. You are wrong. That well-known scientist, Professor Goltz, of Strasburg, tells us that it was "marvelous and astonishing" to find that a dog which had served for some seven experiments, whose hind quarters were completely paralyzed and *whose breasts he had cut off*, was still capable of maternal anxiety and love; "she unceasingly licked the living and the dead puppy,

and treated the living puppy with the same tenderness that an uninjured dog would manifest."*

Or suppose some Washington vivisector in one of the Government laboratories desires, out of scientific curiosity, to repeat the atrocious and perfectly useless experiments of that distinguished scientist, Professor Mantegazza. His problem was to create intense pain and at the same time to compel the creature to keep motionless in an attitude that would not interfere with its breathing. The ingenious scientist devised two methods of accomplishing his end, "either by exasperating the pain, so that its influence overcame the action of the muscles of motion, or by planting sharp and numerous nails through the soles of the feet in such a way as to render the animal nearly motionless, because in every moment it would have felt its torment the more acutely." To exasperate the pain he invented a machine, which he aptly called "a tormentor." With it, he explains, "I can take an ear or a paw and, by turning the handle, squeeze it beneath the teeth of pincers. I can lift the animal by the suffering part. I can tear it or crush it in all sorts of ways." One experiment was on a guinea pig nursing its young. A rabbit, after two hours' torment and a few moments' rest, has nails stuck into its feet in such a way that "a pain much more intense" than in some previous experiment is produced. Two little creatures are subjected for two hours to the tormentor, then "larded with long, thin nails in their limbs." They "suffer horribly, and, shut up in the machine for two hours more, they rush against each other and, not having the strength to bite, remain interlaced, with mouths open, screaming and groaning."†

All these experiments, extending over a year, were conducted, he tells us, not with repugnance, not with dislike, but "*con molto amore*"—with extreme delight. We do not

* Pfluger's Archives, Vol. IX, p. 564.

† Fisiologia del Dolore, di Paulo Mantegazza, pp. 101, 106, 107, etc.

mention these experiments as examples of the average investigations going on in laboratories; doubtless they are extreme instances. The point we desire to make emphatic is this: if such experiments as these of Mantegazza and Goltz can be performed to-day in Washington laboratories, free from any restriction or criticism of any sort; and if, notwithstanding their daily performance, the men at the head of the various vivisecting laboratories could sign memorials to Congress, asserting that "so far as we know, no *cruel* experiments have ever been made in this District;" if all this is possible, then all these denials of cruelty—of cruelty as the world understands it—are absolutely valueless. For certainly if these experiments are not cruel, there is no cruelty in scientific research.

Well, in the first place, just such experiments are entirely possible in any of the Government laboratories of Washington, if, in the opinion of the scientific vivisector at the head of such laboratory, they are "properly conducted." The only law applicable to such experimentation is the act of February 13, 1885 (23 Stat., 302), which says:

SEC. 15. Nothing in this act contained shall be construed to prohibit or interfere with any properly conducted scientific experiments or investigation, which experiment shall be performed only under the authority of the faculty of some regularly incorporated medical college, university, or scientific society.

What is there in this law that would prevent, in Washington laboratories, any number of repetitions of the experiments of Mantegazza and Goltz? Mantegazza has for some time contemplated a visit to this country. Is there a vivisector in Washington who dares to put himself on record that, according to the ethics of the laboratory, this physiologist's investigations were "improperly conducted?"

Secondly, the "cruelty" of such experiments could be denied. One of the leading scientific societies of Washington defines cruel experiments as those in which "there is an unjustifiable infliction of pain." What, to a vivisector, is an

unjustifiable infliction of pain? It is the infliction of more pain than is necessary for the success of the experiment. "Cruelty" as defined by six vivisectors of Harvard University, "is the intentional infliction of *unnecessary* pain." But who is to judge how much or how little pain is "necessary?" Who is to decide whether the subjection of the animal to prolonged torture is of the slightest value? Who, according to the scientific societies of Washington, should be the supreme and only judge of the vivisector? *The vivisector himself!*

You say that this is impossible? You can not believe that any scientific society would so juggle with a question of right or wrong as to make the morality of an act depend solely on inclination of the person who does it? Incredible as it may seem, that is precisely what has been done, and that position constitutes to-day the principal difference between the AMERICAN HUMANE ASSOCIATION and the various scientific societies of the United States. We say that Congress should by law stamp its disapproval of wanton and infamous experiments such as those of Mantegazza and Goltz. "No," say the advocates of free vivisection, "let the vivisectors alone decide what they may do." In the report to Congress from which quotation has been made, there appears a statement signed by the leading vivisectors of the United States. "As to whether or no, under given circumstances of research or teaching, an experiment involving pain should be performed, *is a matter which should rest with the responsible expert by whom, or under whose direction, the thing would be done*" (p. 60). To that declaration of a vivisector's right to be above all criticism or control, we find the names of Daniel E. Salmon, of Charles Wardell Stiles, of Surgeon-General Sternberg. "We believe that those engaged in scientific investigation are the best judges of the necessity for experiments made by them, . . . and of the methods to be employed," says the PHILOSOPHICAL

SOCIETY OF WASHINGTON (p. 133). What is this but to justify the above experiments of Mantegazza and Goltz?

The joint commission of the scientific societies of Washington affirm that those engaged in vivisection investigations "are the best judges of the character of experiments required, and of the necessity for using anaesthetics" (p. 130). The reader is horrified, perhaps, at some of the experiments herein described; but we have only touched the outer edge of the infamy which stains the record of so-called scientific research. Yet it is all permitted, sanctioned, and approved by the scientific societies of Washington, if only it is done by a scientific vivisector! According to the new ideal of scientific morality, the only person in this universe who has the right to say whether any vivisection is right or wrong, cruel or otherwise, is the man who performs it! "Unnecessary and offensive in the highest degree would it be . . . to attempt to dictate or control *how, and by whom, and for what purposes and under what conditions . . . experiments shall be made*" (p. 135). To that horrible sentiment, unanimously approved by one of the great associations of professional vivisectors and their friends, is attached the name of Surgeon-General Sternberg. And now, if opportunity existed, we should like to ask Members of Congress if they distinctly understood that all this denial of cruelty in the laboratories of the District of Columbia, so earnestly made, so solemnly asserted, was put forth with the mental reservation that nothing a vivisector might do would ever be "cruel" unless he called it so himself? Did you fancy that hidden in high-sounding phraseology was the claim that the vivisector alone is qualified to pronounce upon the moral quality of his own actions? Of what value are all their denials of cruelty? Sanchez shall tell us: "*A man may swear that he never did such a thing, though he actually did it, . . . while the words that he employs have no such sense as would discover his meaning.*"

IV. Is the Utility of Vivisection Exaggerated?

Notwithstanding the opinion of that eminent surgeon, Lawson Tait, of England, that "nothing whatever has been gained by vivisection,"* it has always seemed to us more probable that in certain directions, vivisection within limitations is sometimes of such practical and potential utility as to justify its use. But in their eagerness to prevent the slightest degree of Government supervision in the District of Columbia, is it true that certain scientists have made claims of usefulness far beyond the actual truth? One sees nothing of the kind in European countries. There, the idea of utility to humanity as a reason for vivisection is laughed at. Says Professor Hermann, of Zurich University: "The advancement of knowledge, and not utility to medicine, is the true and straightforward object of all vivisection. Science can afford to despise this justification with which vivisection has been defended in England," and he might have added, "in the United States." But public sentiment in this country at present will not sanction the torment of animals unless behind it is the claim of utility or benefit to humanity. Has this claim been pushed, even by men of scientific training, beyond the limits of scientific truth? To those unacquainted with medical phraseology it is diffi-

* The late Prof. Lawson Tait, F.R.C.S., one of the most brilliant surgeons of this century, not only affirmed that vivisection was useless, but also declared that it led to erroneous conclusions. In a letter to the *Birmingham Daily Post*, Dec. 12, 1884, he says:

"Like every member of my profession, I was brought up in the belief that by vivisection had been obtained almost every important fact in physiology, and that many of our most valued means of saving life and diminishing suffering had resulted from experiments on the lower animals. I now know that nothing of the sort is true concerning the art of surgery; and not only do I believe that vivisection has not helped the surgeon one bit, but I know that it has often led him astray."

cult to make evident such exaggeration; but the task at least shall be attempted.

Perhaps the most imposing array of names attached to any memorial to Congress in regard to vivisection is that of the ASSOCIATION OF AMERICAN PHYSICIANS, a body which embraces in its membership, as before pointed out, some of the best known experts in vivisection in the United States. There, in company with leading physicians, are professors and teachers from every quarter; and experimenters such as Sternberg and Vaughan, Meltzer and Flexner, Ernst and Councilman, Adami and Wood, lift in unison their protesting voices against any hindrance to their methods or any supervision of their work. They are men of science, trained in the exactitude which science is supposed to instill. What do they tell us of the benefits which have resulted from vivisection during recent years? We may be sure in so important a document nothing has been omitted which by any possibility could be claimed.

"To mention only a few of the results obtained within recent years by animal experimentation, attention is called to the discoveries which have revolutionized surgical practice by the introduction of antiseptic methods of treatment, which have rendered infrequent the occurrence of childbed fever, which have made it possible to prevent the development of hydrophobia after the bite of rabid animals, which have furnished an efficacious method of cure of the otherwise incurable disease, myxoedema, and which, by the antitoxin treatment, have greatly lessened the fatality of diphtheria" (p. 135).

Now, admitting that experimentation has helped to teach surgery the infinite importance of the exclusion of germs by the most absolute cleanliness; and that in other directions, along lines of experimentation in nowise prevented by the limited regulation which we advocate, experiments are throwing light on other matters—admitting all this,—are the claims here made supported by facts? It may

be questioned. Here in America we have no national system of registration of deaths such as exists in every other civilized country on the globe, and we can not appeal to any national statistics of our own land. We may be sure, however, that any improvement in way of medical or surgical treatment wherever devised, is at once utilized by the physicians and surgeons of Great Britain, and that if such wonderful discoveries have been made as are claimed above, we shall find evidence thereof in the annual reports of English mortality.

1. Is it due to animal experimentation that results have been obtained "which have rendered infrequent the occurrence" of puerperal fever? THE ASSOCIATION OF AMERICAN PHYSICIANS so affirms. On the contrary, it can be proven:

(1) The basis of our knowledge concerning this disease was due to observations in hospitals; and not to animal experimentation.

(2) The disease is not yet "infrequent," judging by the statistics of a nation's mortality.

When the history of medical practice shall one day be written, there is no page we would more willingly have blotted out than that which relates to the causes and treatment of this terrible scourge. It is not only that for twenty centuries medical science was absolutely ignorant of the principal cause of this malady, and that the treatment only added to horror and increased mortality; the tragedy is that the physician himself was, in so many instances, the source of infection. One shudders at the contemplation of the slaughter that went on year after year in the great hospitals of great cities, in Europe and America as well, while medical practitioners, instead of bringing assistance, were often spreading the causes of death throughout a community.

To whom came the first glimmer of truth regarding the causes and prevention of this scourge of maternity? Was it some Mantegazza, bending with delight over his crucified vic-

tins? Was it a Goltz, watching agony mingled with maternal love? To none of these came the truth. It was to a young man who, in 1847, was an assistant in the Lying-in Hospital at Vienna, that medical science owes not only the first teaching of the real facts, but, as Lusk puts it, "a large part of what is now the current doctrine concerning the nature and prevention of puerperal fever."* Because Semmelweis pointed out that the awful scourge was due, not to an "inscrutable and mysterious Providence," but to the carelessness of physicians and their ignorance of the necessity of surgical cleanliness, his discovery was received with ridicule; he was hated and despised in his lifetime, and he died, Lusk tells us, "with no other reward than the scorn of his contemporaries." To-day justice is rendered to his name; and although he did not see the whole truth, although experimentation, acting upon his theory, has broadened our knowledge in many directions, it was primarily to his observations in hospitals, and not to any researches in the laboratory, that the beginnings of all we know regarding the methods of prevention were first brought to light.

Nor is it yet scientifically true that puerperal fever is "infrequent," if we test infrequency not by individual experience or by the records of this or that hospital, but by the mortality of an entire nation. When one considers the terrible mortality which prevailed in the large lying-in hospitals, up even to a quarter of a century ago, it would be impossible that the recognition of the value of surgical cleanliness should not make evident its influence in lessening the disease. In Bellevue Hospital, New York, for example, the rate of mortality from this disease in relation to confinements was, in 1872, more than *one hundred times as high* as that which prevailed during the same year through-

* Lusk's *Science and Art of Midwifery*, pp. 653, 654.

out England and Wales.* The fact that such awful mortality as this has been decreased gives no warrant for the claim that the disease is now infrequent. What has been the experience of England before and since the discoveries to which the Association of American Physicians makes allusion?

During thirteen years, from 1860 to 1873, in England and Wales the death rate of puerperal fever to each 10,000 births varied annually from 13 to 20, only once reaching the highest figure. This was during the period long before any knowledge of antisepsis. Coming to our own time, we find that from 1883 to 1896, inclusive, when methods pertaining to antisepsics were in full sway, there was but one year in which the mortality rate from this cause was as low as 20—the highest rate during the earlier period. The rate for 1893 was twice as high as during any of the eight years, 1860-1863 and 1866-1869, and with but one exception, higher than any time in thirty years. Even in 1896, the mortality from puerperal fever was actually higher than at any time during the period 1860-1873—a quarter of a century ago! Let us compare four years of English experience.†

ENGLAND.	1877.	1878.	1892.	1893.
Total births -----	888,200	891,906	897,957	914,542
Deaths from puerperal fever -----	1,444	1,415	2,356	3,023
Rate of mortality to each 10,000 births-----	16	16	26	33

* Lusk tells us that "in the year 1872 puerperal fever destroyed 28 women of 156 who were confined in the Bellevue Hospital" (p. 692), or 18 per hundred of the women confined! It was only 17 per 10,000 births in England, the same year, 1872.

† All English statistics quoted in this paper have been extracted directly from the reports of the Registrar-General of births, marriages, and deaths. None have been copied from other authorities.

To check any possible source of error, let us compare the foregoing facts with the statistics of London for the same four years.

LONDON.	1877.	1878.	1892.	1893.
Total births	128,092	129,765	132,328	133,062
Deaths from puerperal fever -----	221	195	347	394
Mortality rate per 10,000 births ..	17	15	26	30

These statistics are peculiarly interesting and valuable. Do they support in the slightest degree the assertion of the ASSOCIATION OF AMERICAN PHYSICIANS—made without a word of proof—that the occurrence of puerperal fever has been “rendered infrequent?” Are they not, on the contrary, absolutely contradictory of that claim? Almost a double death rate in a great nation and a great city, and yet the assertion of infrequency? What, we may well ask, is the use of a scientific association,—what is the value of its testimony, if, when scientific facts are so easily accessible, it can not tell us the truth?

2. Have recent experiments “made it possible to prevent the development of hydrophobia after the bite of a rabid animal?”

Taking all the facts into consideration, there is reason to believe that in some cases a certain degree of real immunity is produced, although the evidence is by no means sufficient to release one from the duty of doubt. The failures are very many; and the whole treatment is little more than an immense experiment upon the human race, the results of which are yet to be summed up. But what has this to do with the bill before Congress? All such experimentation as that of Pasteur is permitted by the proposed measure. And how few of us remember the almost infinite rarity of hydro-

phobia as a cause of death compared with other causes of mortality. Take an instance: in England and Wales during the year 1896, and again during 1897, the deaths reported by the Registrar-General as due to vaccination were six times as many as those due to hydrophobia!*

3. We are told that experiments "have furnished an efficacious method of cure of the otherwise incurable disease, myxœdema." Possibly this is true. But the disease is of such exceeding rarity that it is not even described in any but the most recent medical works, and there is nothing in the bill before Congress that would have prevented the alleged discovery.

4. And finally it is said that experimentation has led up to the antitoxin treatment, which has "greatly lessened the fatality of diphtheria."

If it took centuries of experience to determine the uselessness of the lancet and of other methods of treatment so generally in vogue but a little time ago, it is not easy to perceive how the value of this new method of treatment can be absolutely determined until, after many years' trial, it shall be seen that the actual mortality from this disease has steadily decreased during a number of years in each country where it is tried. All statistics based upon the number of "cases" concerning an alleged remedy in which there is a commercial interest, should be viewed, at least, with suspended judgment. Says Dr. Herman of Brooklyn: "Until antitoxin brings down the diphtheria death-rate to a point

* At the hearing before the Senate Committee at Washington, Feb. 21, 1900, Dr. Mary Putnam Jacobi seemed to think that this statement of infrequency was hardly accurate. At the close of her remarks, the Chairman, Senator Gallinger, inquired of her how long she has been engaged in the practice of medicine? "Since 1872," was the answer. "And in that period, (nearly thirty years) how many cases of hydrophobia have you met with?" "Why, I haven't seen any," was Mrs. Jacobi's reply. The Chair made no comment.

lower than it ever was before and keeps it at that point in every place, it must be considered a failure.”*

Now, no fact is more certain than that antitoxin has failed to meet this test. In Boston, in Baltimore, in St. Louis, in Philadelphia,—as Dr. Herman points out,—there were years before the introduction of antitoxin during which the mortality-rate, based upon population, was lower than during other years since its use. In St. Petersburg, the deaths were 378 in 1893, and in 1897, after antitoxin was introduced, the deaths from diphtheria rose to 1,905. The antitoxin treatment in England, so far from lessening the mortality of the disease, has been wholly unable to prevent its vast increase. During five years (1877-1881, inclusive) when antitoxin was wholly unknown, the deaths from diphtheria to each million population of England and Wales were 111, 140, 120, 109, and 121, or an average, roughly, of about 120 per year. How was it after the introduction of antitoxin? The corresponding mortality for 1895 became 260, for 1896 it rose to 292, and in 1897 it was 246—more than double the mortality of certain years when antitoxin was unknown. The Registrar-General, calling attention to the subject, says that with only two exceptions “the death-rate referred to diphtheria alone in 1896 was higher than in any previous year since 1861.” Even if we take the death-rates of diphtheria and croup together, the mortality of 1896, the Registrar-General tells us, has been exceeded only seven times in thirty-three years. Let us glance for a moment at the actual number of deaths from diphtheria and croup in London during two years in which antitoxin has been in use (1895-96), and contrast the mortality they exhibit with that which prevailed only a few years ago, when it was entirely unknown.

* The failure of Antitoxin in the Treatment of Diphtheria,” by J. Edward Herman, M.D., p. 5. See MEDICAL RECORD, May 27, 1899.

London, England.—Deaths from diphtheria and croup, in periods of two years, before and after the introduction of antitoxin.

	1890.	1891.	1895.	1896.
Deaths from Diphtheria.....	1,382	1,433	2,350	2,664
Deaths from Croup.....	505	339	135	136
Death-rate from Diphtheria, per 1,000,000 population	331	340	535	599

During the three years 1895, 1896 and 1897, when antitoxin was in use, the death-rate from diphtheria per million population in the city of London was more than three times as high as that which prevailed during seventeen years from 1865 to 1881, inclusive. In figures like these taken from Government reports, where is the conclusive evidence of that vastly lessened fatality produced by antitoxin, which the ASSOCIATION OF AMERICAN PHYSICIANS has claimed? Even granting that claim, the fact would have no pertinency as an objection to the bill before Congress, which distinctly permits all the experimentation by which antitoxin was discovered. Not a single discovery of any value to humanity, coming from any physiological or pathological laboratory anywhere in the world, during the past quarter of a century, would have been prevented by the legislation that is asked for the District of Columbia. Do the interests of scientific advancement require the suppression of this truth?

In one sense that is the question to-day. All these exaggerations of utility, these petty evasions, these cunning tricks of equivocation and suppressions of the truth,—can we possibly regard them as an honor of science? Could any more saddening disillusion come to those who love learning and who yet cherish faith in the honor of their fellow men, than the conviction that scientists are given to paltering with veracity; and that whenever personal interests are touched, their word cannot be believed?

There is a sphere of activity, no doubt, where honor is unknown. But above that lower world of fraud and pretense, there must be a region of purer and diviner air, where higher ideals are cherished, where truth is held sacred, where falsehood is supremely scorned. If men of science as a class have apparently been far too trustful of their vivisecting brethren, too willing and eager to vouch for their statements, it may be well that they learn by experience the necessity—even here—for scientific doubt. Perhaps the time is ripe for some new expression of the scientific creed. We are quite sure that the great body of scientific workers would therein protest as leading articles of faith, that Science, rightly understood, means only the simple truth; that intentional deception is always a dishonor; and that the sacred cause of learning can never be permanently advanced by exaggeration or deceit.

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